



THE ANALYST.

JANUARY, 1894.

PROCEEDINGS OF THE SOCIETY OF PUBLIC ANALYSTS.

THE usual Monthly Meeting of this Society was held on December 6th at the rooms of the Chemical Society, Burlington House. In the absence of the President, Mr. Otto Hehner occupied the chair.

The minutes of the last meeting were read and confirmed.

The following gentlemen were proposed as members: Dr. J. Duncan, St. Petersburg; Percy Andrews Ellis Richards, A.I.C., Charing Cross Hospital, W.C.; and Robert Walter Oddy, F.I.C., Laboratory, Walterhouse, Toad Lane, Rochdale.

The following gentlemen were duly elected members: J. Kear Colwell, Great Russell Street, W.C.; and H. H. B. Shepherd, Upper Clapton, N.E.

The Chairman (Mr. Hehner), referring to the death of Mr. R. H. Davies, late Secretary to the Society, said: As a Society, and as members of the profession of analytical chemists, we have suffered a great many losses, but on few occasions have we been so deeply touched as on the present one, when, as you know, we have to lament the death of one of our Secretaries, Mr. R. H. Davies. All of us had been aware that he had been ill, hopelessly ill, for many months past, and the most sanguine amongst us could not have expected that he would have been spared very long. He died at Bournemouth on November 16th. Mr. Davies was not a very old member of the Society; he has been a particularly active and prominent member, both in his private capacity and as secretary. All of us who came into intimate contact with him can have nothing but pleasant and affectionate recollections of him. His opinions were decided and sound, he was possessed of a remarkably clear intellect, and as a public analyst he was as popular as a public analyst could be. By those of us who met him as a friend he will never be forgotten. The Council, by virtue of one of our bye-laws, have to-night filled up the vacancy created by the death of Mr. Davies, and have unanimously recommended Mr. Bevan, who I need not introduce to you, to fill the vacant post. He has kindly consented to act until the ballot-papers electing the new Council are opened at the Annual Meeting in January, when I do not doubt he will be found to have received the suffrages of the members. I have, therefore, to ask Mr. Bevan to take his place as one of our Hon. Secretaries.

Mr. Bevan said: I should like to thank you for the honour done me, and to assure you I shall do the utmost for the benefit of the Society at large and any of its members. I need hardly say how deeply I personally have felt the loss of our friend Mr. Davies. He was in every way a genuine friend, one whose opinion was always at the call of any member who chose to ask for it, and I can only feel it to be the more difficult to follow him; but I trust, with your indulgence, to do my best.

Mr. Cassal: I think, in view of the great loss which we deplore—for it is a very great loss to the Society, and one which will be felt for many years—that it would be a gracious and proper act if the Society itself were to pass a vote of sincere sympathy with Mrs. Davies and her family. This is a little more than a mere formal letter of sympathy, officially sent as from the Council. Having regard to the circumstances, I would suggest that a vote of sympathy be sent from this meeting of the Society.

The Chairman: I will take upon myself to propose that a letter of sympathy be sent by the Secretaries on behalf of the Society to Mrs. Davies on the occasion of the death of Mr. Davies. This, I am sure, will not require seconding.

On the motion of Mr. F. H. Perry Coste, seconded by Mr. Cassal, Mr. Chattaway and Mr. Kitto were re-elected auditors.

Dr. Dyer announced that the Council had nominated Dr. W. A. Tilden, the present President of the Institute of Chemistry, for election at the next meeting as an hon. member (applause).

Papers were then read on "An Improvement in Richmond's Milk Scale," by C. E. Cassal and B. H. Gerrans; and on the "Leffmann-Beam Method of Fat Estimation in Milk," Part iii., by H. D. Richmond and L. K. Boseley, and "On Vinegar," by E. Collens. The publication of these is unavoidably held over.

In the absence of the author, Dr. Dyer read the following paper:

ON THE ESTIMATION OF BEEF-FAT IN LARD.

By W. F. KEATING STOCK, F.I.C.

A QUALITATIVE process for the detection of beef-fat in lard is described in *THE ANALYST* for 1888 (p. 70). Its authorship is ascribed to Dr. Belfield, of Chicago. Probably every public analyst is acquainted with this test. Roughly, it consists in making a nearly saturated solution of a suspected sample in ether, and allowing this solution to stand in a test-tube loosely plugged with cotton-wool until separation of stearin occurs, after which the stearin crystals are examined microscopically, and the presence or otherwise of beef-fat is deduced from the crystalline forms observed. The whole procedure as set forth in *THE ANALYST* appears to be simplicity itself, and it is exceedingly simple when the operator has to do with a large proportion of beef-fat, but it becomes far otherwise when quantities of 5 per cent. to 7 per cent. are in question. Under certain circumstances it is possible to overlook the adulterant entirely.

The fact that there was no known method for the direct quantitative determination of beef-fat in lard was brought prominently before my notice in November, 1892, when a considerable number of samples of lard were submitted to me for analysis—all of which were normal as regards iodine-absorption, melting point of fatty acids, density at 99° C., and temperature-rise with strong sulphuric acid, but whose physical characters gave strong presumptive evidence of the presence of a solid fat which was not lard. Tested by Belfield's test for beef-fat as described above, good results were got in a few cases, but in others the presence of beef-fat was recorded as "doubtful," the crystallized ether-deposits failing to yield crystals having sufficiently well-defined

beef-fat forms. This naturally led to experiment, and I sought first the temperatures at which beef-stearin and lard-stearin *began* respectively to separate from ethereal solutions of the same degree of concentration. I found that whilst beef-stearin began to deposit at 17°C ., lard-stearin began to deposit at 14°C . I also found that the presence of lard and beef-fats in the same solution did not sensibly affect these temperatures. I satisfied myself further that, when Belfield's test failed with small proportions of beef-fat, it did so because the undesirable concentration of the ethereal solution resulted in a simultaneous deposition of lard and beef-stearin, and that interference, and not separation, was the consequence.

After making a large number of experiments, I selected 13°C . as the temperature at which I could best deal with commercial lards, and I adopted a *fixed* ether volume seven times greater than the volume of melted fat on which I operated. This, of course, involved the use of stoppered tubes instead of tubes plugged with cotton wool. I further recognised the necessity for bringing the solutions down to 13°C . as slowly as was compatible with convenience, and I fixed twenty-four hours as a time-limit. I may explain that the temperature of 13°C . was selected because it was obviously a guarantee of the deposition of any beef-stearin which might be present if lard-stearin was allowed to deposit, since, as shown above, the deposition temperature of beef-stearin is 3°C . higher than that of lard-stearin—as determined by myself. At this point I was struck by the idea of employing standard mixtures of lard and beef-fat, and, by means of comparison, obtaining quantitative results. The following process was the outcome of this idea :

PROCESS FOR THE ESTIMATION OF BEEF-FAT IN LARD.

This process depends upon the comparative insolubility of beef-stearin in ether at a temperature of 13°C . It is subject to modification according to the melting-point of the sample under examination. It must be employed with the discretion born of experience, and the procedure cannot be departed from in any degree if the author's results are to be criticised.

The requisites are six 25 c.c. graduated test mixers fitted with glass stoppers. A supply of ether, sp. gr. $\cdot 720$.

- “A.” { Pure commercial lard melting at 34° to 35°C .
Beef-stearin melting at 56°C . (pressed beef-fat).
“B.” { Pure commercial lard melting at 39° to 40°C .
Beef-fat melting at 50°C .

A set of standard mixtures of “A” and a set of standard mixtures of “B” are prepared. They must contain 5, 10, 15 and 20 per cent. of beef-stearin and beef-fat respectively. The “A” set is used for samples melting at 33° to 39°C .; the set “B” is used for samples melting at 39° to 45°C .

The melting-point of all samples must be taken by the capillary tube method. The tubes must stand twenty-four hours after filling.

Suppose a sample which is to be tested for beef-fat melts at 34°C ., 3 c.c. of the melted sample are run into one of the 25 c.c. test mixers, dissolved in 21 c.c. of the $\cdot 720$ ether, and placed in a vessel of water at 20° to 25°C . Three c.c. of each of the “A”

series of standards are then dissolved in ether in exactly the same way. The five tubes are then cooled down to $13^{\circ}\text{C}.$, and allowed to stand at that temperature or a lower for twenty-four hours. At the end of that time the apparent volume of deposit in each tube is noted, and this observation gives an immediate clue to the condition of the sample. The ether is poured off from the tubes as far as possible, and 10 c.c. of fresh ether at $13^{\circ}\text{C}.$ added in each case. The stoppers are inserted and the tubes well shaken. When the deposit has settled out the operation is repeated. The whole contents of the tubes are now transferred to weighed shallow beakers. The ether is carefully run off (it may be used by repeated decantation to clear the tubes), but no fresh ether must be used. The deposits are dried at $40^{\circ}\text{C}.$ for ten to fifteen minutes. The beakers are cooled and weighed. The standard weight nearest to that of the sample is used as the factor by which to calculate the beef-fat present.

The actual presence of beef-fat must be determined by the microscope. For this purpose a few particles of the dry residue are placed on a slide, moistened with alcohol and covered. *Very* moderate pressure must be applied to the cover, and the slide is viewed by a 1-inch objective and a C ocular. This power may, of course, be varied. With practice, however, there is no difficulty in recognising beef-stearin with the naked eye. The values of the standards being once determined, they need not be kept. The figures are constant if time, temperature and volume are constant.

Note.—The foregoing was written when it was perfectly easy to maintain a constant temperature of $13^{\circ}\text{C}.$ for days. Since that time the author has had recourse to an ice chest, and he sees no objection to the tubes standing at a lower temperature than $13^{\circ}\text{C}.$, provided that decantation and washing are carried out at $13^{\circ}\text{C}.$ exactly.

Recent experiments have proved that by lowering the temperature of the filled tubes to about $16^{\circ}\text{C}.$, and allowing them to stand two hours in water kept at that temperature by occasional additions of fragments of ice, and then standing them in water at about $10^{\circ}\text{C}.$ over-night in a non-conducting apparatus, made of wooden boxes and dry sawdust, just as good results were got as when a constant temperature of $13^{\circ}\text{C}.$ was available.

I may say here that I communicated the working details of this process to a number of my professional friends, and have received from them many assurances of approval.

I soon found that in my own practice the "B" set of standards met my wants, and I therefore give their values in the following table. But I would advise every operator to prepare the standards and determine their values for himself.

TABLE I.—COMPOSITION AND VALUES OF THE "B" SET OF STANDARDS.

Composition.		Melting-point Centigrade.	Ether-washed deposit in milligrammes.	Difference of ether-washed deposit.
<i>Beef fat.</i>	<i>Pure lard.</i>			
5	95	40.2	43	
10	90	41.0	81	38 milligrammes
15	85	41.6	122	41
20	80	42.4	181	59 "

The beef-fat melted at $50^{\circ}\text{C}.$

The lard melted at $39^{\circ}\text{C}.$ and gave 8 to 11 milligrammes of ether-washed deposit.

The necessity for two sets of standard mixtures may not be obvious at first sight, but when it is seen how variable a substance commercial lard is, and when it appears that there are at least four kinds of commercial beef-fat, the necessity for *trying* to meet some of the problems likely to arise from these facts will likewise come in sight. The following table will perhaps best illustrate my meaning :

TABLE II.—TABLE OF DIFFERENCES FOUND IN PURE LARDS AND BEEF FATS.

Origin of sample.	Melting-point Centigrade.	Ether-washed deposit in milligrammes.
No. 1. Leaf lard (rendered in laboratory)	45·8°	146
No. 2. Belly lard " " "	39·0°	11
No. 3. Back lard " " "	34·5°	None in 24 hours
No. 4. Leaf lard (Liverpool merchant) ...	45·5°	114
No. 5. Soft lard " " " " ...	34·0°	6
No. 6. Hard lard (Hull merchant) ...	42·5°	83
No. 7. " " " (rendered by Mr. E. Bevan) ... " " " " ...	41·2°	90
No. 8. Pressed beef-fat (trade) ...	56·0°	Not determined.
No. 9. " " " " " " ...	53·0°	" "
No. 10. Refined beef-dripping (trade) ...	43°	" "
No. 11. Hard beef-suet (rendered in laboratory) ... " " " ...	50°	" "

The ether-washed deposit (E.W.D.) is, of course, the total lard-stearin deposited in each case, and it appears, as might have been expected, that the amount of E.W.D. is greatest where the melting-point is highest. But it further appears that when the stearin exceeds a certain proportion it is deposited in a rapidly-increasing ratio (compare Nos. 1 and 4). It will be seen also that although the melting-point of No. 2 is considerably higher than that of most samples of commercial lard, it gives only 11 milligrammes of E.W.D., whilst Nos. 3 and 5, with still lower melting-points, give none and 6 milligrammes of E.W.D. respectively. *These facts enable me to state the following proposition, That no normal sample of lard melting below 39° C. would give by my process an E.W.D. of more than 11 milligrammes.*

This, at any rate, has been my experience in applying my process to 80 samples of lard sent to me in my public capacity. Of these 80 samples, 32 have been found to contain beef-fat. In 27 of these cases prosecutions have been instituted, and in 25 convictions have followed. In two cases the summonses were withdrawn in consequence of conflicting certificates from the analysts at Somerset House. In both of the latter cases these gentlemen failed to find the adulterant. In one of them I sent the ether-washed deposit round to Mr. Alfred H. Allen, Mr. Otto Hehner, Dr. Alfred Hill, and Mr. E. W. T. Jones, all of whom certified to the presence of beef-fat stearin. Further comment is unnecessary.

By way of showing how the process has been applied by me, I append the results of the analysis of 21 of the adulterated samples :

TABLE III.

Origin of sample.		Melting-point.	E.W.D.	Result reported.
1.	Liverpool merchant ...	34·6° C.	50 milligrammes	7% beef-fat
2.	" " ...	34·6° "	59 "	7% "
3.	" " ...	39·0° "	52 "	6% "
4.	" " ...	45·5° "	222 "	24% "
5.	Hull " ...	40·2° "	73 "	9-10% "
6.	County sample, No. 68	34·6° "	65 "	8% "
7.	" " No. 64	40·3° "	102 "	12% "
8.	" " No. 394	37·6° "	36 "	5% "
*9.	" " No. 455	38·8° "	56 "	7% "
10.	" " No. 440	36·2° "	38 "	5% "
11.	" " No. 260	38·6° "	52 "	7% "
12.	" " No. 37	35·6° "	34 "	5% "
13.	" " No. 7	36·7° "	18 "	2·5% "
14.	" " No. 4	36·7° "	39 "	5% "
15.	" " No. 18	41·8° "	32 "	5% "
16.	" " No. 342	42·6° "	178 "	20% "
17.	" " No. 340	41·8° "	125 "	15% "
*18.	" " No. 8	37·6° "	36 "	5% "
19.	" " No. 409	40·8° "	54 "	7% "
20.	" " No. 265	41·5° "	86 "	10% "
21.	" " No. 156	44·2° "	151 "	18% "

The samples marked * are those in which the Somerset House analysts failed to find beef-fat.

Some of the figures I have given doubtless demand explanation. For example, it will be seen that in the table of pure lards and beef-fats (Table II.) I have shown that in two cases (Nos. 1 and 4) the E.W.D. rises to 146 milligrammes and 114 milligrammes respectively, and the question which is naturally prompted by intelligent criticism is: How would you proceed in a case where a sample, having the quality of Sample No. 4, had received the necessary addition of beef-fat to bring its melting-point and E.W.D. to about that of No. 1? Would the whole E.W.D. not be reckoned as due to beef-fat? Such a case demands careful consideration; but I have had the question to answer under very trying conditions when I was dealing with test samples, of the history of which I knew absolutely nothing. Due regard to the following points has enabled me to overcome any difficulty of this kind. 1st, The melting-point of the sample argues either a highly stearinized lard or a large proportion of beef-fat. 2nd, The *initial deposition temperature* serves as a most excellent guide to the proportion of beef-fat. 3rd, The bulk and character of the deposit, which forms before the temperature of 14° C. is reached, confirms the second observation. If the deposit at 14° C. is bulky and granular, it is due largely to beef-fat. 4th, The proportion of plumose crystals in the E.W.D. as seen under microscopic examination. If only 3 or 4 plumose crystals per field are visible, beef-fat cannot be present in large proportion. 5th, The E.W.D. is recrystallized from successive portions of 5 c.c. of ether, cooling down gradually to 14° C., until lard-stearin is no longer present in appreciable amount. The E.W.D. may now be looked upon as beef-stearin, and may be calculated into beef-fat of 56° C. melting-point. This was the course pursued

in Sample No. 4, Table III., where it will be seen that the melting-point is that of leaf lard nearly, whilst the E.W.D. is 222 milligrammes as against 146 milligrammes for leaf lard.

Direct experiment has shown me that neither cotton-oil, palm-nut-kernel-oil, nor cocoa-nut-oil interfere in any sensible degree with the deposition of the crystals of beef-stearin.

It would, I think, hardly be fair to those who may wish to employ the process I have described, if I left unnoticed some confirmatory tests which may be applied to the E.W.D. in cases of doubt, or if I failed to refer to some points of the microscopical manipulation.

It sometimes happens that the crystals of beef-stearin are thick and ill defined. In such a case recrystallization from 5 c.c. or so of ether is resorted to, and in my hands this has always effected the desired end. It is important that the solution should be slowly cooled as before. Petroleum spirit, having a boiling-point of 80° C., or under, may be employed for the same purpose, and gives good results.

The crystals should be mounted with care, and should not be subjected to any grinding motion in pressing down the cover-glass. It is, in my opinion, unwise to employ high powers for the examination of the stearin. The field is thereby restricted, and to my mind there is no substantial gain in definition after 100 diameters is reached. On the other hand, much may be learned by the use of the polarizer with selenites. Practice in these matters rapidly makes perfect. Some of the operators in my laboratory can speak to 5 per cent. or 7 per cent. of beef-fat in a lard by simply examining the E.W.D. with a pocket-lens.

DISCUSSION.

Dr. Dyer wished to know why beef-stearin, and not mutton-stearin, was always talked of. Was not the heavier portion of mutton fat used for the adulteration of lard, or was beef-fat only used for that purpose? He did not remember ever to have heard of a case where mutton-fat was alleged to have been added to lard, but he believed that both beef and mutton were used in the manufacture of margarine.

The Chairman observed that pig's fat was used in margarine only to a small extent, not mutton-fat. He had obtained a considerable number of deposits which were not beef-stearin and not pig-stearin. He (the Chairman) had had deposits which he was convinced were cotton-stearin. Mr. Stock had evidently gone much deeper into the question than any other public analyst in this country. The question was a very difficult one, so difficult that if any chemist had been asked, but a few years ago, what hope there was of distinguishing between pure pig-stearin or lard and beef-stearin, he would have answered that it was hopeless to expect to do so.

Mr. Allen said he had had the advantage of seeing the isolation of beef-stearin carried out in Mr. Stock's own laboratory, and he went through the experiments with him, Mr. Stock personally explaining every detail of the process. He spent much time over the microscope, pointing out the distinctions to be observed in various

mixtures of beef-fat with pig-fat or lard. He (Mr. Allen) was pleased with what he learned from Mr. Stock, and he was convinced he was very careful in coming to his conclusions, though he (Mr. Allen) confessed he had not succeeded in mastering all Mr. Stock's reasoning. The process was by no means a difficult one, and, as Mr. Stock had pointed out, fully justified his conclusion that beef-stearin was insoluble in ether at 13° C., and they would have next to no loss by crystallizing. He would recommend that the members of the Society interested in lard should give the process very careful attention. Mr. Stock he knew to be an admirable manipulator and of a very critical turn of mind, and he would not put before the Society these details without the strongest reason for his belief in them. He (Mr. Allen) had no doubt that when they had more experience they would be able to follow the manner in which Mr. Stock arrived at the figures, some of which at present appeared anomalous. It was a very gratifying fact that Mr. Stock's authorities had got convictions in most instances, and where they had not secured convictions, it was because Somerset House could not find beef-fat in samples which he (Mr. Allen) had personally examined and fully satisfied himself contained it.

Mr. Cassal had worked somewhat extensively at this subject, and could say that there was but little difficulty in detecting beef-fat so long as the work was done carefully and the procedure mentioned by Mr. Stock was followed. But he would be glad to have some further information from Mr. Stock as to his method of drawing quantitative conclusions. It was plain that Mr. Stock drew his conclusions from a consideration of all the data obtained, and that he especially relied upon the melting-points; but more definite instructions were requisite, and some further explanations were necessary in regard to some of the apparently abnormal figures for ether-washed deposit. Quantitative conclusions drawn from such figures as these would be, he presumed, settled mainly by the melting-points.

Mr. Allen said petroleum ether, as a crystallizing agent, gave a far better result than when ordinary ether was used.

(Conclusion of the Society's Proceedings.)

DISCUSSION ON THE VINEGAR QUESTION.

(Held at the Meeting, Nov. 1st, 1893.)

Mr. Alfred H. Allen, in opening the discussion, said: We are all very well aware of the interest and importance of the Vinegar Question at the present moment. I have already delivered myself of one or two opinions in print on this matter, added to which I have a great deal to say which is already stale to many of us who are reading various reports in the papers; but if I were not to mention, at any rate cursorily, the different points of interest in connection with the matter, I might deprive you of certain pegs on which to hang your arguments subsequently, and therefore I propose to go shortly over the history of the question from the commencement. In the first place, I may point out how many instances there are in the English language of words getting corrupted from their original meaning. Thus, geometry

does not now mean the measurement of the earth. We use the term trigonometry or mensuration to signify this. It is the same with vinegar, which we all know is derived from *vinaigre*, sour wine; but it has been diverted from its original application. No one would now contend that the term vinegar should be limited to wine which has undergone acetification. In certain parts of England the vinegar with which we are more familiar, made from malt or similar materials, is called "alegar." I contend that alegar is a perfectly parallel word to vinegar—that is, vinegar is sour wine and alegar is sour ale; and although ale does not make the best vinegar, alegar is a perfectly legitimate term, and it seems a pity it is not generally adhered to. By "vinegar" we do not now usually mean wine vinegar, though I have an analysis before me of a genuine wine vinegar. In many parts of the country by "white wine vinegar" is simply understood acetic acid. In that sense it has been perverted from its original meaning in a remarkable manner. A few months ago there was a considerable difference of opinion among analysts as to what should be included in the definition of vinegar. In the same manner we have no difficulty in saying that wine vinegar should be inside the definition, but there are certain articles which are very difficult to define. However, we have been relieved from that to a large extent by the decision at the Birmingham July Quarter Sessions, when the learned Recorder held distinctly that wood-acid was not vinegar. Therefore, we have now a knowledge of what is *not* vinegar, but the Court did not lay down exactly what vinegar was. The Recorder had before him a case of vinegar which was said by the public analyst to contain 70 or 80 per cent. of pyroligneous acid, and the defence admitted the accuracy of his certificate, while the actual analysis showed that this statement by no means over-estimated the proportion of pyroligneous acid present. The Recorder decided that wood-acid is not vinegar, although there were witnesses examined before him who contended that vinegar was anything which contained acetic acid of such purity and at such dilution as to be capable of being used for food. There is a great deal to be said for that view, but a great deal more against it. If we refer to our dictionaries, and works of special character bearing on the subject, we find almost invariably that "wood-vinegar" is distinguished from "vinegar" pure and simple, and hence it is not surprising to find that at Birmingham vinegar was defined in the following manner: "Vinegar is an acid liquid produced by the alcoholic and acetous fermentation of a vegetable juice or infusion." That is, perhaps, a little too stringent a definition, because there may be substances not coming within it which may nevertheless be held to be vinegar. When we once attempt to draw the line, the difficulty arises. Thus there is a German product made from distilled spirit, a product which varies to a certain extent in the details of its manufacture; this distilled spirit is taken and submitted to the acetic fermentation. By the fermentation acetic acid is produced, and acetic ether and other secondary products are also formed. That part of the process is such as is characteristic of the manufacture of a genuine vinegar. But distilled alcohol cannot be regarded as "a vegetable juice or infusion." A similar product is made by taking a malt wort or other saccharine liquid, and subjecting it to the acetous fermentation, and when that is complete, or nearly complete, a certain amount of alcohol is added to it, which in its turn is allowed to acidify, and gradually,

by repeating this treatment, a product is ultimately obtained containing 12 to 13 per cent. of acetic acid. Are we to regard this product as "vinegar," or should it not be sold always as "spirit-vinegar," or by some similar name, indicating its peculiar origin? In Scotland, distilled vinegar is preferred to undistilled, and is far more commonly met with. Are the people there not to have distilled vinegar, therefore, if they prefer it? or if they do, are they always to be obliged to ask for it as "distilled vinegar"? There are certain vinegar brewers who, recognising the importance of reducing the amount of albuminoids, and certain other constituents in the vinegar, distil a portion of their vinegar; then they add water to the undistilled portion, to obtain the weaker qualities, and add the distilled product. They claim that a vinegar so made will keep better, but at the same time they take away some important analytical distinctions between genuine vinegar and an article containing wood-acid. This practice has prevailed pretty extensively with certain vinegars, but it only applies to the very lowest grades of strength. That is a subject, therefore, which distinctly calls for our attention. It is very difficult, indeed, to draw the line as to what should be considered as genuine in such instances, and such mixtures are extremely liable to be mistaken for articles composed of fermentation vinegar mixed with wood-acid. In consequence of the acknowledged difficulty of distinguishing fermentation vinegar, where such additions had been made, from an article containing wood acetic acid, many of the inspectors who have submitted samples to analysts have been inclined to ask for samples of "malt vinegar." There are not a few manufacturers who advertise their products as malt vinegar, but "malt vinegar" is very rarely asked for by the public. They ask for Smith's vinegar or Brown's vinegar, giving the name of the manufacturer; but I believe it very rarely happens that people go and ask for "malt vinegar," and the result has been that a demand has been created for malt vinegar by the inspectors acting under the Sale of Food Act. There is no difficulty whatever where you have an adjectival prefix to the term vinegar. If a manufacturer sells his product as "pure wood vinegar," or "pure unfermented vinegar," which is the description adopted by a certain vinegar "faker," I do not know that we can complain of it. But I think that it is a dangerous practice to say that a vinegar contains a certain amount of "pyroligneous acid," unless the presence of tarry products makes its origin absolutely certain. If my information is correct, the term pyroligneous acid is applied in commerce to a particular article which has not had the advantage of being purified from the tarry products which are usually associated with crude pyroligneous acid. When purified by conversion into a lime or soda salt I believe it ceases technically to be called "pyroligneous acid," and is denominated "acetic acid." I have in my mind a case where an analyst, called for the defence, said that a vinegar did not contain pyroligneous acid because there were no tarry products present. That was probably perfectly correct. The term pyroligneous acid may indicate the origin of an acetic acid, but unless tarry products are distinctly recognisable it is in my opinion a mistake to use the term. I believe it is the fact that vinegar is now never, or hardly ever, made from malted grain alone. I believe there is practically no vinegar now made solely from malted barley; there is always a mixture of unmalted barley, or other unmalted grain with it. Public analysts have been inclined to say, "Yes,

that is all right ; we understand by malt vinegar, a vinegar made from grain where the starch has been converted into saccharine matter through the action of diastase, and, seeing that the diastase in malt is greater in quantity than is necessary to convert the starch of that malt, by converting the starch of unmalted barley into sugar you are merely making the diastase do its full duty." But if, accepting the above reasoning as correct, we give as a definition of " malt vinegar," " vinegar made from saccharine matter which has been converted from starch purely through the action of diastase," we are immediately met with the question, " Why limit the converting agent to diastase?" The diastase converts the starch into a mixture of maltose and dextrin. Why should we be limited to diastase? Why may not we use saliva? Why should a manufacturer be limited to diastase if he can accomplish the same result with another ferment or by another substance capable of producing hydrolysis? Why should not he convert the starch into malt sugar and dextrin by means of acid? I have here cut, from the *Brewers' Journal*, a whole series of advertisements of various preparations which the vendors call " malt." The general practice in the brewing trade appears to be to regard as " malt " any saccharine material produced from grain, either by a process of cooking, by the action of diastase, or by conversion with acid, the result being the same, though the tools employed are different.

It is interesting to consider the definitions of malt given in our standard dictionaries. Thus Johnson describes malt as " Grain steeped in water, and fermented ; then dried on a kiln."

Ogilvie's Imperial Dictionary, which is a modern one, says that the word malt is derived from the Anglo-Saxon *meltan*, to melt, to dissolve, to digest, to cook ; and defines malt as " grain, usually barley, steeped in water and made to germinate, the starch of the grain being thus converted into saccharine matter ; after which it is dried in a kiln, and then used in the brewing of porter, ale, or beer, and in whisky distilling."

The new edition of Webster's Dictionary defines malt as " barley or other grain, steeped in water and dried in a kiln, thus forcing germination until the saccharine principle has been evolved. It is used in brewing and in the distillation of whisky."

In other words, two of our modern standard dictionaries stick to the definition of malt as meaning sprouted grain, and do not give the term the extended meaning which beer-brewers do, and vinegar-brewers would like to, put upon it. My own feeling is that the public analyst stands between the public and the vendors of these articles. What does the public understand by malt? An intelligent man desiring information would turn to a dictionary and would say : " I see, it is made from grain which has sprouted." Therefore, the public would expect, if they asked for malt vinegar, that it would be made from such sprouted grain. On the other hand, we cannot at present recognise any chemical distinction between acid converted and diastase converted starch. But it does not follow that there no difference exists. It is quite possible that there are distinctions which we have hitherto failed to discover, just in the same way as there are distinctions between the different qualities of whisky and other fermentation products which are at present inap-

preciable to the most careful analyst. As chemistry advances we may be able to ascertain in what these distinctions consist. The manufacturer says that the term malt means to melt, and that whether they melt or dissolve it by means of acid, or diastase, or saliva, it makes no difference. Besides, it is argued that this diastase is, after all, only a solvent, and when it is done with you want to get rid of it, as the albuminoids are very objectionable. I believe vinegar manufacturers do attempt to keep down the albuminoids and use processes more or less suited for eliminating a portion of the phosphates and other substances which are apt to facilitate secondary changes. It is quite possible that vinegar keeps better and is otherwise materially improved if the conversion of the starch is effected by the agency of acids instead of by diastase. I am stating here the arguments of intelligent technical brewing men, because they are arguments which will most probably be used in important cases in which public analysts will be concerned in the future. I have already stated that, to my mind, whether we go to the dictionary definition or not, we should protect the public by requiring that the conversion should be by diastase; but, on the other hand, if it be held that we are drawing the rein too tight by requiring that malt vinegar should be made through the action of diastase, then it becomes a question of what is desirable. We have in vinegar brewing a legitimate industry, carried on with a certain amount of scientific knowledge which may be very greatly improved on, but in which there is a certain amount of skill and ability, and the vinegar brewers are producing a good merchantable article, which, although it may not satisfy the requirements of public analysts, is in a very different position to the product of the vinegar "fakers," who, by the aid of a second-hand tub, and a few barrels of acetic acid, set up as vinegar "manufacturers," and produce an article which is very different in character from a genuine brewed vinegar. Vinegar varies as much in quality as does wine, and there is a taste to be cultivated in vinegar just as there is in wine. I should like as far as possible, legitimately with our duties as public analysts, to see the trade in vinegar encouraged, instead of our giving countenance to German manufacturers who import their product as "acetic acid"; and then, having coloured it to represent vinegar, sell it in opposition to those who make vinegar by true fermentation processes. I am sorry to think that sometimes the manufacturers, for whom I have individually a great respect, have been rather hardly dealt with by public analysts who have not always taken into consideration the whole of the circumstances of the case. It has become necessary for us to discriminate between the different kinds of vinegar, and the question arises, How are we to do it? Unfortunately, we are at sea for want of definite information as to the composition of vinegars of various characters; and, seeing that these different kinds of vinegar overlap at each stage, and manufacturers are perfectly justified in improving their vinegar by taking out the constituents which are liable to cause an objectionable change in their product, however inconvenient such practices may be to public analysts, it follows that we are necessarily in a very great difficulty.

The limited quantity of vinegar ordinarily brought to a public analyst is another practical difficulty with which we have to contend. I am sorry to say that some of my inspectors have, on occasions, brought me one-third of a pennyworth. There is no object in purchasing so small a quantity as half a pint, or even one pint. It

would be much more sensible to buy a barrel, as was actually done in one instance at the suggestion of the analyst. Really the question of using 100 c.c., or 1,000 c.c., is not to the purpose if we can ascertain something which we could not ascertain with smaller quantities. The succinic acid of vinegar I hope to be able to find useful to us in the future, although I have nothing very favourable to report at present, and am now trying if the glycerin is of any value as a proof of origin. Alcohol always exists in a well-made fermentation vinegar, for manufacturers stop the process before the acetification is complete. I have some instances of vinegar which has diminished in strength to the extent of fully 1 per cent. of acid in six months. If the alcohol is all destroyed the change is likely to be much more rapid afterwards, since, in the absence of other food, the vinegar-fungus feeds on the acetic acid previously formed. A well-made vinegar should contain alcohol, not only for its keeping purposes, but also because you have a gradual formation of acetic ether, just the same as in wine after keeping. We might distinguish the fermentation vinegar in that way. At the same time we must remember that it is very easy to add alcohol in imitation of a fermentation vinegar. The German manufacturers put acetic ether into their acetic acid with a view of making it as like vinegar as possible. There is a considerable amount of solid extract in fermentation vinegar, but when you are dealing with a mixture containing pyroligneous acid the quantity is very much less. The solid matter varies very much according to the perfection of the fermentation, and affords an indication of some value, though not so great as the amount of ash, which does not vary to a great extent through the fermentation. The proportion of sulphuric acid will afford some information as to the probable use of glucose, and so on. We have carried out the estimation of nitrogen on a large number of samples, and find it to give us a very valuable criterion. First of all, you cannot get grain vinegars without a large amount of nitrogen; for although the manufacturers do attempt to remove a certain amount of the nitrogenous matters, there is always a large amount left. In estimating the total nitrogen by the Kjeldahl method, we did not add sulphuric acid direct to the vinegar, as is the custom of some analysts, but have evaporated the vinegar to dryness, or at any rate to a syrup, before adding the acid. 25 c.c. of vinegar is a convenient quantity to employ. The nitrogen found can then be calculated to its equivalent of albuminoids by the usual factor; but I am much inclined to think that much of the organic nitrogen of vinegar exists as peptones or similar soluble forms. In one case where we made the experiment one-tenth of the whole content of nitrogen was found to consist of ammoniacal salts. The proportions of all these constituents must necessarily vary with the strength of the vinegar. A wort which originally contained 12 per cent. of sugar and other solids will necessarily contain more nitrogen, ash, phosphates, etc., than a vinegar which originally contained only 7 per cent. of sugar. Therefore, it is extremely desirable to adopt Mr. Hehner's plan of calculating the various constituents upon the original solids of the vinegar; 60 parts acetic acid are theoretically produced from 90 of glucose, and hence, if we multiply the acetic acid found by 1.5, we obtain the amount of sugar from which that acetic acid was derived.

Adding to the figure thus obtained the total extractive matters still contained in the vinegar, we obtain a number representing the "original solids" of the wort.

Thus, if a vinegar contain 5.2 per cent. of acetic acid and 2.8 of extract, the original solids will be $5.2 + 2.6 + 2.8 = 10.6$. If the vinegar itself contained 0.08 of nitrogen, the original solids will contain—

$$\frac{0.08 \times 100}{10.6} = 0.75 \text{ per cent.}$$

In this manner one can eliminate the differences caused by variations in the strength of various samples of vinegar, and reduce the results to a kind of common denominator. As a matter of fact, the loss of acetic acid in the process of manufacture averages some 30 per cent., so that the proportion of original solids calculated in the above manner is always below the truth. Hence a nearer approximation to accuracy would be obtained by multiplying the acetic acid by 2.25, instead of 1.5, before adding the extract. But the change would involve confusion, and hence I have adhered to the mode of calculation originally suggested by Mr. Hehner.

During the last few months upwards of 200 samples of vinegar have been analysed in my laboratory. Many of these were samples submitted under the Sale of Food Act, but a considerable number were typical samples of vinegar submitted by leading manufacturers. It is useless to distinguish the samples of "vinegar" from those of so-called "malt-vinegar," as many of the latter were found on analysis to be very far from justifying such a description.

I must leave for some future occasion the question of the range of variation in composition possible in pure vinegars of various origins, but give on the opposite page the figures representing, according to my experience, the composition of certain typical vinegars of definite origins.

Of the samples of which the quantitative analytical results are given above, the first four, with the possible exception of A, I believe to be genuine grain vinegar, brewed from a mixture of malted and unmalted barley or similar grain, and the starch converted entirely by the action of diastase. E is the average analysis of the first seven vinegars of Hehner's table (*THE ANALYST*, xvi. 82), described by him as "partly to my knowledge undoubtedly made from malt only, partly made by the best London manufacturers."

F and G are genuine vinegars brewed from a mixture of malted and unmalted barley, with the addition of sugar.

H and I are vinegars manufactured chiefly from rice, the conversion being effected by the aid of sulphuric acid. They contained respectively 0.138 and 0.143 grammes of combined SO_3 per 100 c.c.

J and K were manufactured from sugar, with perhaps a little malt in the former case.

L is the vinegar condemned by Dr. Hill, which formed the subject of the appeal before the Birmingham Recorder. The figures have been communicated by Dr. Hill.

M is a sample of very pale vinegar, made by mixing distilled vinegar with a little of the same vinegar undistilled. It possessed a very appetising smell and taste.

(To be continued.)

Sample Mark.	A	B	C	D	E	F	G	H	I	J	K	L	M
<i>Specific gravity</i> ...	1.0205	1.0170	1.0228	1.0160	—	1.0130	1.0185	1.0190	1.0160	1.0104	—	1.0070	1.0104
<i>Per 100 parts of vinegar.*</i>													
Acetic acid ...	6.61	6.39	5.26	4.86	4.23	5.22	5.82	5.58	5.70	3.51	4.92	4.70	7.00
Total solids ...	2.81	2.67	3.96	2.31	2.70	1.56	2.45	2.98	2.09	1.52	1.76	0.21	0.10
Ash ...	0.55	0.34	0.40	0.47	0.34	0.30	0.39	0.30	0.43	0.27	0.278	0.04	0.015
containing:													
Alkalinity as K_2O	0.102	0.091	0.118	—	0.024	0.03	—	0.013	—	0.080	—	Trace.	Trace.
Phosphoric acid ...	0.066	0.077	0.093	0.057	0.105	0.064	0.041	0.017	0.024	0.010	0.016	0.009	None.
Nitrogen ...	0.120	0.099	0.095	0.099	—	0.052	0.097	0.104	0.062	0.014	0.016	—	0.002
Albuminoids ...	0.756	0.624	0.598	0.624	—	0.328	0.611	0.655	0.390	0.088	0.103	—	0.013
"Original solids" ...	12.73	12.26	11.85	9.60	9.35	9.39	11.18	11.35	10.64	6.81	10.02	7.26	10.60
<i>Per 100 parts of original solids:</i>													
Ash ...	4.32	2.78	3.37	4.92	3.64	3.20	3.49	2.64	4.04	3.94	2.77	0.55	0.14
Phosphoric acid ...	0.52	0.63	0.79	0.60	1.16	0.68	0.37	0.15	0.225	0.14	0.16	0.120	None.
Nitrogen ...	0.95	0.816	0.80	1.03	—	0.56	0.87	0.93	0.582	0.206	0.16	—	0.019
— Albuminoids ...	5.98	5.14	5.04	6.49	—	3.53	5.48	5.86	3.670	1.30	1.03	—	0.120

* The figures are grammes per 100 c.c. of the vinegar.

THE PROXIMATE COMPOSITION OF BUTTER.

BY H. DROOP RICHMOND.

At the end of 1890 Dr. Vieth read a paper before the Society, in which he gave in abstract the results of 267 analyses of butter, and described the method used for these (ANALYST, xvi, 1). Since that time samples have been regularly examined by Dr. Vieth up to March, 1892, and since then by myself by the same method.

In view of the interest now attaching to the question of the amount of water in butter, I have thought it desirable to supplement Dr. Vieth's paper by giving the figures obtained up to the present date. I have nothing to add to his remarks on the methods of churning and preparing butter for the market, nor to the details of the method of analysis given in his paper.

In Table I. are given the results of the whole of the water determinations made by Dr. Vieth and myself; this is a simple extension of that given by Dr. Vieth (*loc. cit.*), and is in the form adopted by him.

TABLE I.

Percentage of Water.	Number of Samples.	Per cent.
7-8	2	·4
8-9	8	1·4
9-10	18	3·2
10-11	37	6·6
11-12	76	13·6
12-13	95	17·0
13-14	210	37·5
14-15	88	15·7
15-16	21	3·8
16-17	3	·5
17-18	2	·4
	560	100·1

A reference to the table given in Dr. Vieth's paper shows that, although the number of samples is more than doubled, the percentages of water found in butter is practically unchanged, the third column (percentages of total samples) differing by only 1·5 per cent. as an extreme.

These 560 samples include various kinds of butter, among them being English, French, Danish, Swedish, German (Kiel), and Australian butters; many of them were churned at Bayswater, practically speaking, under the eyes of Dr. Vieth and myself, while the others were samples of butter on the market. As it is a well-known fact that butter loses water during the handling it receives in commerce, I have thought it desirable to divide these samples into two classes—English butters, which were analyzed soon after churning; and foreign butters, which had stood a sea-voyage and been otherwise handled.

TABLE II.—ENGLISH BUTTERS.

Percentage of Water.	Number of Samples.	Per cent.
7-8	2	1.4
8-9	5	3.5
9-10	13	9.1
10-11	17	11.9
11-12	36	25.2
12-13	33	23.1
13-14	23	16.1
14-15	9	6.3
15-16	4	2.8
16-17	1	.7
	143	100.1

TABLE III.—FOREIGN BUTTERS.

Percentage of Water.	Number of Samples.	Per cent.
7-8	—	0
8-9	3	.7
9-10	5	1.2
10-11	20	4.8
11-12	40	9.6
12-13	62	14.9
13-14	187	44.9
14-15	79	18.9
15-16	17	4.1
16-17	2	.5
17-18	2	.5
	417	100.1

The history of the English butters is known, and they were made with a reasonable amount of care, as commercial butter; nothing is known of the origin of the foreign butters, and these may have included samples purposely mixed with water, or which, by carelessness in manufacture, contained an excess of buttermilk. Dr. Vieth has expressed the opinion that 16 per cent. of water should be considered as the maximum allowable in butter, and considering the above tables, I can only give my heartiest approval of this view.

Out of the whole 560 samples, but five of them contained more than 16 per cent. of water. One of these, a Swedish butter, contained a great excess of curd, and had a cheesy taste, and was evidently, from this fact alone, a very carelessly-made butter; another sample, the only English one above 16 per cent. (contained 16.49 per cent.), was churned at a very high temperature, owing to the weather being very hot, and a supply of ice temporarily not obtainable. Of the other three samples, one was a Danish butter, and the other two Kiel butters.

In my opinion, the adoption of 16 per cent. as the highest permissible limit will inflict no hardship on honest traders, as it is quite high enough to include butters in

the churning of which slight errors of judgment or mistakes—to which even the best dairyman is liable—have been made, while the adoption of higher limits will but open the way to fraudulent addition of water.

WATER ANALYSIS—THE INTERPRETATION OF RESULTS—*Continued.*

BY F. WALLIS STODDART.

(Reprinted from "*The Practitioner.*")

THE one flaw in both limits is that neither has any better foundation than general experience, which has not invariably supported them. There is a wide difference, however, between these definitions of purification, and that based simply upon the completion of nitrification. Is either limit to be accepted as satisfactory? And if so, which? Convincing answers to these very important queries are to be obtained only by direct experiment as to the conditions of nitrification, and a consideration of recorded cases of the spread of disease by polluted water. What is required is fairly exact information as to the extent both of space and time within which nitrification can be completed, and the chances of survival of pathogenic organisms under the same condition.

(a) With regard to the depth of soil, and the length of time within which nitrification can be completed, two methods of investigation are possible: the artificial production of nitrification, and the examination of natural waters under known conditions.

Now, in connection with the first method some very remarkable experiments were recorded by the Rivers Pollution Commission in their First Report. In these experiments sewage was filtered through soils of various kinds contained in glass cylinders, and the very noteworthy result was obtained that, using a soil in which the process of nitrification was thoroughly established, no less than 97 per cent. of the nitrogen of London sewage could be converted into nitrates by percolation through only five feet of soil.

There are, however, some points in which these experiments might be improved, in the light of more modern knowledge as to the character of the nitrification process. For instance, the sewage was poured on the soil twice every twenty-four hours, thus alternately waterlogging the soil and leaving it comparatively idle. On the other hand it might be objected that the filtrate was collected only once a week, and that the process of nitrification might be, and probably was, proceeding all the time in the water. Still it is sufficiently remarkable that in these experiments raw sewage was converted into a water which compared favourably as regards organic matter with that supplied to the Metropolis at that date (1869) for domestic purposes.

I have for some time adopted a similar arrangement for class demonstration, using columns of coarsely-powdered chalk about two inches in height contained in glass tubes. By carefully seeding these tubes with the appropriate organisms, it is easy to produce quite a copious formation of nitrous acid from ammonia, or nitric from nitrous acid, by allowing the proper solutions to drop slowly upon the upper surface of the chalk.

I have more recently extended these experiments in order to get quantitative results. My filter consists of a column of coarsely-powdered chalk twenty-four inches in height contained in a one-inch condenser tube. A drop arrangement is made so that the water trickles slowly but continuously over the particles of chalk, thus representing as nearly as may be the conditions existing in the superficial air-charged layers of soil. Nitrification is established by first dropping a little putrid urine on the chalk and then passing a solution of urea (0.01 per cent.) until the effluent is strongly ammoniacal. A polluted well-water in which the nitrous fermentation predominates is then substituted for the urea, and when nitrites appear freely, a strongly nitrating well-water is used in the same way. For some reason I have succeeded better with well-water than with extracts of soil. If now a solution of ammonium carbonate, containing 1 part nitrogen in 10,000 and representing sewage, is allowed to flow through the tube, a very constant formation of calcium nitrate is observed. By suitably forcing the rate of flow, nitrite and ammonia may readily be made to appear in the filtrate; but a very considerable quantity of solution can be completely nitrated daily.

My first experiment with this filter was made with a very badly polluted well water, which, however, was perfectly bright and contained no suspended matter; it was passed through at the rate of about one pint in twenty-four hours, and afterwards at double this rate. The results given in the following table expressed in grains per gallon were obtained.

Here we have a water which would be condemned unhesitatingly as rankly polluted, and beyond question dangerous to health, so altered in composition as exactly to resemble what I have frequently seen described by analysts who confine themselves to a determination of the "ammonias" as "water of remarkable organic

	Original water.	Filtered water.	
		1 pint per diem.	2 pints per diem.
Saline ammonia	·252	·0003	None
Albuminoid ammonia	·018	·0023	·0056
Nitrogen as nitrates and nitrites ...	4.61	5.77	5.48
	(Nitrites abundant)	(No nitrites)	(No nitrites)
Chlorine... ..	8.8	8.8	8.9
Total solids	112.0	106.0	107.0
Phosphoric acid	Very heavy trace	...	Very heavy trace
Total hardness	57.0	...	56.5
Permanent hardness	33.0	...	39.0
Oxygen absorbed in fifteen minutes ...	·084	...	·034
Oxygen absorbed in four hours ...	·112	...	·059
Colour	Deep yellow-brown	...	Yellow-green

purity," and this by a natural process which could not possibly be imagined to ensure the removal of infective matter. The second sample yielding a quart per diem represents about the maximum amount of impurity which could pass muster without

remark, such water being frequently described within my experience as "slightly contaminated with vegetable matter (absence of saline ammonia), and somewhat highly charged with mineral salts of an innocent character." Now, this interpretation is plainly entirely beside the truth, and a proper appreciation of the results other than the "ammonias," chlorides, and total solids, even if the effluent were largely diluted with pure water, would at once lead to a recognition of the dangerous character of the supply.

Two interesting points are incidentally brought out in this experiment—the increase of permanent at the expense of temporary hardness, and the abundance of phosphates in the filtered water, of which 70 ccm. under suitable treatment gave a decided precipitate with molybdate, thus showing that there is no foundation for the assertion that carbonate of calcium and phosphates are incompatible in drinking water.

The water was travelling through the filter at the rate of two feet in about forty minutes, a rate probably considerably in excess of that at which subsoil water generally moves.

It may be objected, however, that nitrification was already far advanced in this water when treated, implying a certain unknown amount of previous natural filtration; besides, it is desirable to know how rapidly raw sewage is affected. I therefore procured crude sewage, and finding one tube did not carry nitrification to sufficient completion when keeping up the same rate of flow, I added another, bringing up the total height of the column to five feet. I may point out that my object in making this modification, instead of diminishing the rate of flow, was to procure sufficient water for analysis without leaving it to accumulate more than twenty-four hours; the same end might be arrived at by duplicating the apparatus, but five feet of soil did not seem an unreasonable distance to assume as generally intervening between well and sewer. The nitrification with this filter improved constantly for weeks, and when the process was stopped the filter was yielding water with the following composition at the rate of one pint per diem:

	Sewage.	Effluent water.
Saline ammonia	3·85	·0013
Albuminoid ammonia	·175	·0158
Nitrogen as nitrates	None	5·99
Nitrites	None	None
Chlorine as chlorides	7·9	7·8
Oxygen absorbed in fifteen minutes	·566	·066
Oxygen absorbed in four hours	1·22	·131
Total dissolved solids	51·0	79·0

The sewage then was converted, by percolation through five feet of coarse soil, into water which if not of completely satisfactory organic purity as judged by the "ammonias" only, is certainly as pure as the well-water which in a previous experiment was improved into a first-class water by further percolation through two feet of similar soil: and if the sewage effluent be supposed to leak into a well of unpolluted water, it may form at least one-third part of the whole supply without

being detected by this test. This experiment then more than confirms the experience of the River Pollution Commissioners—I say more than confirms, because the rate of flow is enormously greater in my form of experiment, one cubic yard of soil dealing more completely with 155 gallons of sewage per diem, than the same quantity does with ten gallons in that of the Commissioners. This is obviously due to the more regular and continuous flow combined with perfect aëration: and it is fair to assume that the process will be completed even more rapidly in the natural soil, especially in that of large towns, where it has been in constant operation for generations.

A simple calculation shows that the flow of one pint of water per diem through a filter one inch in diameter corresponds to a yield of twenty-three gallons per square foot; or in a well twenty feet deep and three feet in diameter, a yield of over 4,000 gallons a day. This is sufficient to account for the fact that the prevailing type of shallow-well-water in large towns is that showing oxidized pollution, and it is only where the source of pollution is in very immediate contiguity to the well that much unaltered sewage is found in the water. In such cases it is commonly the case that other wells in close proximity show no signs of such unoxidized sewage. I give here an instance of two such wells belonging to adjoining houses, which illustrates this point sufficiently.

More direct evidence, however, is afforded by waters contaminated from a source of pollution the position of which is known. The third example in the table is a case in point, where a difference of opinion as to the purity of the water was cleared up

EXAMPLES OF RAPID NITRIFICATION.

	Well to well, 10 yards.		Drain to well, 3 yards.
	A.	B.	
Saline ammonia	·018	None	·0004
Albuminoid ammonia	·020	·005	·0035
Nitrogen as nitrates and nitrites ...	6·92	4·61	4·04
Nitrites	Abundant	None	None
Chlorine	10·8	7·8	15·05
Oxygen absorbed in fifteen minutes ...	·081	·028	—
Oxygen absorbed in four hours ...	·126	·070	0·45
Total dissolved solids	142·0	116·0	103·2
Total hardness	75·0	62·0	40·9
Permanent hardness	51	38·0	—
Appearance, etc.	Good	Good	Good

by the discovery of a choked drain at a distance of about three yards, with evident signs of percolation into the well.

A number of similar examples will be found in *THE ANALYST*, viii., p. 59.

The necessary extent of percolation for the practical completion of nitrification is then limited to at most a few yards, a distance quite insignificant when compared with that which has been shown in many epidemics to fail entirely as a protection against the conveyance of disease.

As to the duration of this process, a very false impression has, I think, arisen

owing to the majority of investigators having carried out their experiments in flasks, whereby the operation is much retarded in consequence of insufficient aëration. Under suitable conditions, as already shown, it occupies only an hour or two with raw sewage.

(b) Few persons, I take it, would rely on filtration under the above circumstances to remove specific organisms by mechanical retention. The experiments of Plagge, Piefke, Koch, and others, have shown that carefully prepared artificial filters can only effect a partial purification when the whole of the conditions are under control, and that no filter can be relied upon to effect even this for an unlimited time. I find that the filter tube above described does produce certain interesting effects upon the various forms of micro-organisms abounding in sewage, but is totally inefficient in preventing the passage of the cholera spirillum. A few drops of a culture introduced into the sewage passing into the filter yielded abundant evidence of the presence of the spirillum in the effluent after an interval of about two hours, and continued to do so for some hours—until, in fact, the filter was dismantled for sterilization.

There are, however, one or two other points worth consideration, in particular the length of time pathogenic organisms can persist in natural waters. A great number of investigators have worked at this question, and their results are conveniently summarized by Professors Frankland and Marshall Ward (*Proc. Roy. Soc.*, li. 183). These results are not very concordant, obviously in part because of varying conditions of experiment. All, however, are agreed that the cholera spirillum will persist in fairly pure water for several days, especially if in vigorous condition, and that probably the typhoid bacillus is even more resistant. I am convinced that this estimate, though abundant for our present purpose, is much understated.

I have been experimenting with the cholera spirillum (as being very sensitive and easily recognised) for some time, and may briefly state here that there is no difficulty in keeping it alive at the ordinary temperature for some weeks in pure and in polluted well-water, and for a still longer time in distilled water and various sterilized saline solutions. I attribute the low estimate of some observers to a want of appreciation of the fact that no culture process probably represents adequately the conditions of the animal body, and that certainly the gelatine plate process is very far from doing so. By cultivating at the body temperature in broth or peptone-salt solution, typical growths may be obtained not only from material apparently exhausted, but also from polluted waters which give hopelessly complicated results with the gelatine plate. However, as nitrification occupies a few hours only, there is no reason to doubt the persistence of pathogenic forms on the score of time.

V. Nor is there satisfactory reason to suspect such antagonism between water organisms and pathogenic forms as to lead to rapid destruction of the latter.

Very little appears to have been done in the direction of allowing pure cultures to grow in the same solution. Professor Marshall Ward, however, shows in the second Report to the Water Research Committee of the Royal Society (*Proc. Roy. Soc.*, liii. 164) that anthrax gets the better of the bacillus fluorescens under these conditions; but as mentioned above, I have found the cholera spirillum persist for many days in both classes of polluted water, *i.e.* waters containing much fresh sewage and those in which nitrification is nearly completed, and in the presence, therefore, of

innumerable organisms of the various kinds naturally present in such water; and further that it will pass through soil in which sewage is nitrifying rapidly without apparent hurt. The only ground, therefore, for suspecting a destructive influence on the pathogenic forms on the part of water organisms being the apparently greater persistence of the former in sterilized than in non-sterile waters, a result which may not unlikely be due in part to defective methods of examination, I conclude that such influence is not a very powerful one, if it exist at all. A great point is made of this antagonism in the recent Report of the Royal Commission on the Water Supply of the Metropolis, but no new positive evidence is adduced in support of the theory.

(To be continued.)

The Determination of Bromine in Urine. A. Nicolle. (*J. Pharm. Chim.*, 1893, xxviii, 298; through *Chem. Zeit.*)—Most processes in vogue for the determination of bromine in urine, in presence of chlorine, are tedious and operose, as, for example, the conversion of the mixed precipitate of silver chloride and bromide into chloride by ignition in a stream of chlorine. The volumetric estimation by titration with chlorine water, and the absorption of the liberated bromine in carbon disulphide, has two disadvantages, viz., the necessity of daily standardization of the solution of chlorine, and the difficulty of discerning the end point. The author therefore adopts Dechau's process, depending on the use of potassium bichromate.

50 c.c. of the urine are evaporated to dryness, with the addition of 2 grammes of caustic potash, and incinerated at a low red heat. The ash is extracted with boiling water, the solution filtered, and the filtrate—which should be colourless, and measure with the washings about 40 c.c.—treated with 10 c.c. of sulphuric acid and poured into a flask containing 20 grammes of potassium bichromate. The flask is connected with a bulb tube containing 20–25 c.c. of a 4% solution of potassium iodide, the apparatus being preferably of glass throughout, or (if rubber must be used) with the rubber connections freed from excess of sulphur by previous extraction with caustic potash. The contents of the flask is heated to boiling for a quarter of an hour, the contents of the bulb tube diluted to 50 c.c. and titrated with sodium hyposulphite. In the presence of iodides in the urine, the aqueous extract of the ash is only neutralized with sulphuric acid and distilled with potassium bichromate, under which conditions the iodine alone is liberated, the bromine being afterwards obtained on adding a further quantity of sulphuric acid and continuing the distillation in the manner already described.

The presence of sulphur in the urine, notably when it contains albumin, may cause trouble by the formation of sulphides, which are dissolved by the extraction of the ash, and interfere with the estimation of the bromine by the action of the sulphuretted hydrogen to which they give rise. This can be met by adding oxalic acid to the aqueous extract of the ash, and boiling until no more sulphuretted hydrogen is given off. When only sulphates in the urine have to be provided for, precipitation of the original liquid with barium chloride suffices. B. B.

NOTE BY ABTRACTOR.—To avoid possible liberation of halogen acids by oxalic acid, the use of succinic acid (*THE ANALYST*, 1893, 255) may be applicable here.

The Determination of the Specific Gravity of Curdled Milk. **M. Weibull.** (*Chem. Zeit.*, 1893, xvii. 1679.)—The author adds a known volume of ammonia to the curdled milk, mixes well, and determines the specific gravity of the mixture. As the specific gravity of the ammonia is known, and no sensible alteration of volume occurs on mixing, the specific gravity of the milk is easily calculated. The following table shows the accuracy of the method when tried on samples the specific gravity of which was taken while they were fresh, and then again after the lapse of some days.

Sp. Gr. Fresh Milk.			Days kept.			Sp. Gr. calculated from that of Milk and Ammonia.
1·0243	...	...	10	...	...	1·0243
1·0243	...	...	10	...	...	1·0243
1·0154	...	...	12	...	...	1·0151
1·0317	...	...	4	...	...	1·0320
1·0283	...	...	4	...	...	1·0280
1·0217	...	...	1	...	...	1·0217

B. B.

The Volumetric Determination of Alkaloids in Drugs. **C. Caspari and A. Dohme.** (*Pharm. Rundsch.*, 1893, xi. 229; through *Chem. Zeit.*)—The conventional method for the determination of alkaloids in drugs consists in extracting them in a state of approximate purity, and weighing the substance thus obtained. The author, in common with other chemists, considers that the approximation to purity usually attained leaves much to be desired, and prefers the volumetric method. His strength of conviction may be gathered from the following dicta:

(1) The volumetric method for determining alkaloids in drugs by means of standard acid is the most reliable and exact method with which we are at present acquainted.

(2) Gravimetric results, which are stated as representing the contents of pure alkaloid, are in most cases erroneous.

B. B.

The Chemical Recognition of Horse-flesh. **W. Brautigam and Edelmann.** (*Pharm. C. H.*, 1893, xiv. 557; through *Chem. Zeit.*)—The method is based on the use of the well-known iodine reaction of glycogen, a body which is a constant constituent of horse-flesh. The finely-divided flesh is boiled with four times its weight of water, and the resulting broth treated with dilute nitric acid, to precipitate albuminoids, and filtered. Saturated hydriodic acid is then added, so that the two liquids remain in distinct layers, and at their plane of contact a red or violet ring forms should glycogen be present. In the event of extraction of the glycogen with water proving inadequate, a solution of caustic potash containing an amount of KOH equal to 3 per cent. of the weight of the flesh must be substituted.

The reaction is said to be characteristic, as it is not yielded by the flesh of other domestic animals.

B. B.